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The Aeronautical Journal.

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NOTICES OF The Aeronautical Society.

Meetings of the Council were held on April 26th and on May 27th, when the following gentlemen were elected Members of the Society:—

Mr. JOSEPH CLARKE, A.I.E.E., Metropolitan Electric Supply Co.
Mr. DAVID M. B. SMYTH, Cuckfield.
Mr. F. W. DICKINSON, Leeds.
Mr. ALFRED WALKER, Bradford.
Sr. D. FRANCISCO INESTA, of Daimiel, Spain.
Dr. F. A. BARTON, Beckenham.

B. BADEN-POWELL, CAPT.,
Hon. Sec.

It has been decided to hold a meeting of the Society on July 28th, at the Society of Arts, when it is hoped that working models will be exhibited by

Dr. Barton—Navigable balloon with aeroplane.
Mr. Smyth—Aeroplane with new form of propeller.
Mr. Broadwell—Vertically lifting revolving aeroplanes.
Mr. Cochrane—Valvular wing action driven by compressed air engine.

The Aeronautical Society of Great Britain.

A meeting of the above Society was held at the Society of Arts, John Street, Adelphi, W.C., on Friday evening the 26th May, under the presidency of Mr. Percy S. Pilcher. With Mr. Pilcher, on the platform, were Captain Baden-Powell, the Honorary Secretary of the Society, and Mr. Lawrence Hargrave, the gentleman so well-known to all interested in the science of Aeronautics, and whose name, in connection with his Flying Machine, has lately been brought very prominently before the public notice by more than one London paper. It was to hear Mr. Hargrave's paper on Kites and Soaring Machines that the Meeting had been convened. There was a large attendance, and previous to the commencement of the Meeting, much interest was shown in the model of Mr. Hargrave's soaring machine and also in his kites, a number of which had been erected in the hall.

In opening the proceedings the Chairman said that he wished to introduce the Meeting to Mr. Lawrence Hargrave (Applause), whose name they had doubtless heard, and who had been in the papers of all nationalities in regard to the most interesting experiments which he had made in kites and kite-flying. Mr. Hargrave, continued the Chairman, has lately obtained some most wonderful results. I am glad he has photographs with him, otherwise we should have great difficulty in believing some of the startling facts which he will tell us, and of which he will be able to produce the photographs. Mr. Hargrave is the modern inventor of what is now called the "Box" kite, which takes its form from an old Chinese pattern. The type has been revived really by Mr. Hargrave, although we continually see reference to it in American papers as an American invention. It is used by a great many American kite-flyers, and I think among others by those who have got high altitudes at Blue Hill Observatory. I will now call upon Mr. Hargrave to address the Meeting.

Mr. LAWRENCE HARGRAVE (who was received with applause) said:—

This box of kites has been brought to London with the hope that they would be

of interest to those who believe that flying and soaring machines will be soon made and used. I noticed in one of our Aeronautical Journals that there was not a specimen of a Hargrave kite in England. Here are some that I can vouch for as being genuine samples; and I will proceed to describe them.

Passing over the endless variety of paper, cardboard, wood, tin, and aluminium kites that have been tried during the last 6 years, many of which have been more or less faithfully illustrated in the Journal of the Royal Society of New South Wales, with the intention that all men should derive equal benefit from their use, we come to the two-equal-celled species as an example of the survival of the fittest.

The kites used by Rotch at Boston are about the same size and proportion as these 40-foot ones, but they are not made to fold up as Blue Hill Observatory has ample stable accommodation.

I hope particular notice will be taken of the various joints and fastenings. I pride myself on making every detail as simple and light as possible. It is unceasing attention to apparent trivialities that insures success.

A good deal of redwood was used for the earlier kites, but now I prefer Oregon. I should not use heavier wood, even if strong straight-grained stuff such as ash was easily procurable.

The spars of the large kite are approaching the dimensions that require tubular constructions of the 8 jack-yards and two fore-and-aft of material.

Much praise has been lavished on bamboo, but I have never seen a sample that I would work into a kite.

The kites are all covered with common calico; only one very small one has been made of silk. I think silk is not suitable for any kites of these sizes; balloon silk would be too strong, and therefore useless weight would have to be carried, and if you used a lighter silk its elasticity would be highly detrimental. Silk might be the best material for the cover of a kite that had about 200 square feet of lifting surface.

Large kites are not to be recommended, they are not efficient per square foot or per pound weight as small ones.

The edges of the calico covers are hemmed and strong fishing line run through. The two covers are rove on a horizontal spar and a board laid in their lower loops. Weights are placed on the board till the fabric is stretched evenly. The pieces of fishing line are then secured permanently, so that when the kite is finished there will be no shivering surfaces.

A measuring tape is passed round each hem before the weights are removed, and the position to obtain the greatest strength per pound sticks are marked. This process ensured exact parallelism and rectangularity of the completed structure.

The continuous diagonal struts boom out the lower jack-yards and the cut diagonals keep the upper corners out, and when the centre tackles are set up the kite is perfectly rigid. All the bevelled ends of the struts are truly cut in a bevel-box so that they fit exactly to their respective seats without any trimming.

The centre tackles of the forward cells transmit all the strain of the flying line when a tandem is used.

The kite with the two main spars parallel horizontally instead of vertically, is a noteworthy structure. It has the disadvantage of requiring more care in unfolding than the other ones.

The action is practically the same as opening four four-ribbed umbrellas simultaneously. On the other hand the furled length is shorter, there are no loose pieces, and the total weight is less.

As to the most economical size of kite to use for any purpose, I think that those with a horizontal surface of 20 square feet are the best, they can be made to weigh one ounce per square foot, and may be depended on to lift one pound per square foot in a 20-mile breeze; that is, one ounce of material lifts fifteen ounces of load.

A late American publication ("Live Steam," January, 1899) states that the Blue Hill kites also lift one pound per square foot in a 20-mile wind.

The American copies of my cellular kites have no diagonal struts, the corners being pushed out by horizontal and vertical pieces of wood. In addition they require to be guyed in every direction by steel-wire stays.

This style appears to me barbaric. Horizontal pieces of wood adjacent to the horizontal or lifting calico surfaces must impair their efficiency. However tightly the calico may be stretched the cobweb of wire stays used to keep the affair cubical must tend to slacken it. Again, if all is perfectly square and rigid before the kite is flown, the moment the wind blows on the calico half the wire stays must slack up and allow the instrument to assume a rhomboidal form.

Elastic slings have been used in America, they add to the complication but keep the kite at a more efficient angle in light winds. Arched ribs increase the lifting power of the kite, and improve the set of the canvas.

The flying of cellular kites is a very simple process, you hold the string in one hand and the kite in the other; watch for a chance, let go the kite and pay out the string. There is absolutely no assistance required till the strain is more than you can hold, and even then by under-running the line the lowest kite of a tandem may be safely landed. A winch, of course, would be a very useful adjunct to a kit of man-lifting kites.

As each kite is added to a tandem the team increases in steadiness, there is no yawing and darting about; therefore fly the smallest one first from a position where the wind is steadiest, then take the line to leeward of a clump of trees, or into a pit, and attach as many more as you require, undisturbed by gusts of wind or flying sand.

When furling a cellular kite, the tackles are eased up first, the struts unseated and folded parallel to the jack-yards, and all the sticks laid together. Then the calico is rolled round the sticks and secured with a gasket.

The compactness of the outfit may be judged from the fact that all these, with a total lifting surface of upwards of 300 square feet, do not nearly fill that box.

The object of elaborating this system of kites was not to enable me to elevate myself to view the surroundings, and, as a matter of fact, I have only been up a few times to an extreme height of 20 feet, but to obtain a stable and reliable set of superposed aeroplanes for a flying machine.

It now remains to make a motor sufficiently powerful to thrust the aeroplanes to windward so as to slack the kite line; and, as many of propeller, the weight, and the tail.

I now come to the question of soaring, and what that wooden thing with the heavy lead weight does.

Some of us believe the form of a soaring bird's wings is such that sufficient power is extracted from a volume of disturbed air moving in a horizontal direction to not only support the bird, but to raise it without the expenditure of any muscular energy.

Of course this is incredible to those who have not watched a soaring bird, and who believe that our text books contain the mechanics of all existing things.

The Aeronautical Society of Great Britain has been good enough to reprint several of my papers on this subject, in which I have endeavoured to make clear how a curve similar to a soaring bird's wing pushes towards the wind that blows on its convex surface.

My mathematical knowledge is insufficient to figure out that the experiments only show what was to be expected. It is impossible to calculate what the result will be till the general formula is approximately known. The formula can only originate from observations more or less skilfully conducted, and must ever be open to modification as knowledge increases. Fortunately, leisure and opportunity have enabled me to produce quite a variety of rigid constructions that have on many occasions soared, and from which I selected the one before you as being most suitable for another experimenter to copy.

Soaring Model.

The soaring kite consists of three parts, the propeller, the weight, and the trail.

It has been found that the propeller is most satisfactorily produced by carving from the solid wood. Inferior articles have been made of tin, aluminium, veneer, and calico.

The curvature and dimensions of this propeller are not supposed to be anywhere near the proportions of what will eventually be the standard, but they give as good results as any I have obtained. The propeller is Lord Kelvin's "Sorting Demon" mentioned in Langley's "Internal work of the wind."

The lead weight consists of 2 hemispheres and a number of discs. These are hung below the propeller in such a manner that adjustments of the quantity and position of the weight in a fore-and-aft direction may be easily made.

Some of the photographs show another method of hanging the weight to the propeller that presents less area for head resistance but has the disadvantage of having no screw adjustment.

The tail acts only as a weather cock. There is no horizontal surface to alter the aspect of the propeller in altitude, this is determined by the position of the weight and its automatic adjustment is still undiscovered.

In order that repeated trial may be made with one of these soaring kites it is absolutely necessary that provision shall be made against breakage. I have used two light poles about 24 feet long standing 48 feet apart on the sand close to the sea, and all the soaring kites I have tried have been hung from the centre of a cord stretched between the pole tops. The hanging line is long enough to prevent the kite striking either the sand or the poles. From this you will see that the kite is free to roam anywhere within a sphere of 40 feet diameter.

The adjustment of the centre of gravity is best made by putting on a slightly excessive amount of ballast, and allowing the kite to swing towards the wind. If the tail lifts, the

weight is too far forward; if the weight is too far aft the kite will seldom pass the plumb position.

It is easier to adjust the amount of ballast for a low than a high angle of soar, because the wind velocity increases with the height; and if the kite gets into the wind for which it is insufficiently loaded, the lead is unable to maintain the propeller at the requisite negative angle.

With a wind velocity of 12 to 14 miles per hour, and a load 1.78 lbs. per square foot, this kite will ascend slowly at an angle of 45° to windward.

With a wind velocity of about 17 miles per hour, and a load of 2.13 lbs. per square foot, it will rise to an angle of 70° or 80° to windward.

Observations of this nature seldom bear immediate fruit, and in the ordinary course of events they must lie buried for an extended period. In the meantime the Aeronautical Society of Great Britain is fulfilling the intentions of its founders, by distributing the seed as widely as possible.

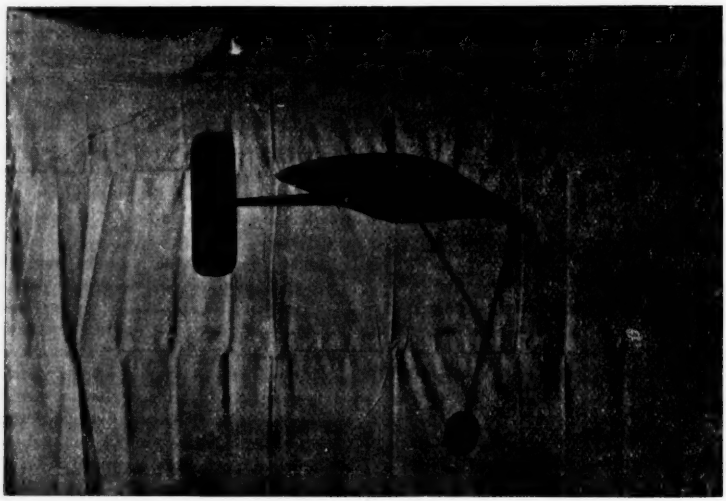
In my papers on soaring, frequent mention is made of a vortex forming in the hollow to leeward of the downward projecting lobe of soaring curves, and that it is this vortex that pushes the wing to windward.

I am now in a position to point to where this vortex can be seen by anyone who wishes.

When a 20-mile breeze is blowing across the top of a steamer's funnel, observe the rolls of smoke pouring out when the furnaces are charged, wait a minute till the smoke thins sufficiently to be almost transparent, and then volumes of smoke of a spherical form will be noticed darting to windward in the centre of the smoke stream from a point about three funnel diameters to leeward; these disappear at the funnel.

Many still speak of this soaring kite as a patent. I have not patented anything and never will. I have not the means or inclination to enter on a campaign with infringers; therefore I rest content with destroying any novelty there may be for ever."

Mr. Hargrave's paper was much applauded. At the close he said that he had a number of photographs and prints, together with copies of papers on the subject which he had brought there for any Member of the Society, or for others, who might wish to forward the objects he had in view. Inspection of the kites would be sufficient to show how they were constructed. They showed the various improvements and methods he had tried and the progress made.



Mr. Hargrave then gave to the Meeting some details of his soaring model, and explained the curve on the under side. The weight, he said, could be adjusted by a screw, and moved backwards and forwards; the weights could also be taken out and replaced by others. He likewise explained his experiments with the machine, which had been conducted on the sea-shore, and as near to the sea as possible, so as to avoid the incline of

the beach; he also gave particulars of the effects of the weight of wind upon his model.

It was only free to go within a sphere of 40 ft. diameter.

The Chairman suggested that, as questions were sure to be asked about it, Mr. Hargrave should take to pieces one of his kites and fold it up, so that the Meeting might see how it was done. Mr. Hargrave willingly complied with this suggestion. He selected what, he said, he considered to be the latest and best of his models, and in the course of a minute or two, with the greatest ease, he had taken it apart and compactly folded it together. To this model there were only four loose pieces.

Mr. Hargrave said that the wood employed was Oregon wood, though he did not know exactly what it was called over here. It was the red-pine, and had turpentine in it.

Mr. Hargrave also folded up one of his other and larger kites.

The best plan for attaching the string was about the back edge of the front sail in a heavy wind, or nearer the middle for light winds. He also showed the Meeting the size and quality of the line used.

The Chairman: Mr. Hargrave has told us of the wonderful soaring feat of this wooden model, but he does not appear, as he said, to account for the "Sorting Demon," as Lord Kelvin called it. I think it would be interesting if Mr. Maxim would give us his ideas of how to account for this wonderful aspiration, or going to the windward—how is it that the machine goes to the windward of its attachment. I know Mr. Maxim has thought very much about it.

Mr. H. Maxim (who was received with applause) said:—"Mr. Chairman, ladies and gentlemen, it was my good fortune during this last Autumn to witness the experiments which were being conducted with Mr. Hargrave's kites at Blue Hill, near Boston, Massachusetts. I should say that that hill is about 500 ft. high, and there is generally a strong wind blowing. They have some very beautiful apparatus for testing the kites there, using steel wire, and I should think they had about three kites on the same line. They told me that on some occasions, when the kites were being pulled down, they would go to the windward, so that they were really on the windward side of the winding station. That, however, did not occur while I was there. I could not catch all Mr. Hargrave said, but if, as I understand it, what he affirms is absolutely true, then the matter is of great importance." Continuing, the speaker gave the result of some experiments as to the proportion of lift to drift, and that on some occasions he had got 18 times as much of the former as of the latter motion. According to Mr. Hargrave's experiments, the quantity of lift in proportion to the drift became infinite. He could not understand it; perhaps Mr. Hargrave could give some explanation of it?

Mr. Hargrave: I am totally unable to explain. I can only speak from my experiments.

Mr. Maxim said that they must understand the weight and load of the kite, and must also compute the angle at which the string was pulling it. As far, however, as actually going against the air was concerned, that was something he was totally unable to comprehend himself. He admitted it was too much for him. (Laughter.)

Mr. Middleton then asked permission to address the meeting, and mounting the platform, explained, more by the use of figures, which he drew in chalk, than by words, how he accounted for the action of Mr. Hargrave's machine. To wit, by calling attention to the fact that as currents of air do not flow generally in straight and horizontal lines, but in more or less wavy curves, *most commonly inclined upwards*, it followed that the explanation of soaring in birds was extremely similar to the mathematical solution of the problem of sailing over the surface of the sea, as the speaker had pointed out in 1890 in his lecture before the Aeronautical Society. And Mr. Middleton added that such mathematical explanation rested on the experimental proof of the *upward* trend of currents of air which was published by Herr Lilienthal in 1889.

That currents or air originally horizontally moving would have impressed on them a vertical component by flowing over highly heated plains or the upward sloping sides of mountains, appeared to the speaker as almost self evident. Still, in physical science it was best if possible to give a proof of the assumed facts on which the validity of such mathematico-physical reasoning rested; and this the speaker repeated he believed was supplied in the matter under discussion by Herr Lilienthal's work. But whether or not Lilienthal was the first to discover the upward trend of these currents of air, one thing was certain, and that was by its admission as a fact the hitherto unexplained, inexplicable or unsolvable problem of soaring became subject to the ordinary laws of mathematical analysis, as would be seen by contemplating the annexed figure or rough diagram, in which the line A represents in direction and magnitude the force with which an elementary current of air impinges on a strip of wing and is deflected without friction or loss of velocity in the direction CD.

For, if we draw from C, the posterior edge of the elementary wing strip, a line CB, parallel and equal in magnitude to the force

produced by the current stream A; and then draw CD of length equal to CB, and tangent to the wing curve at C, it will be seen that the line DB can be looked on as representing the upward reaction of the air against the wing, and the effect thereon would be either progressive and supporting; supporting alone; or retarding and supporting; according to the inclination of this line; or as the point D falls behind; coincides with, or falls before the point E in the vertical line BE. Or in other words according as we turn the deflecting surface of the wing.

Further, Mr. Middleton said he had in past years, spoken to very many people who he believed were competent to reason on these matters, and he thought the points he had put forward could be accepted as a valid explanation of such parts of the observed phenomena as were capable of being rationalized.

However, the explanation given, was not a complete explanation, as it was based on an ignoration of the unknown action of the currents of air impinging on the back of the wing surface. For, it must not be forgotten that the wing was not acted on by the fluid on one side only—as is the case of the vane of a water-wheel—but was wholly immersed in the medium; and most probably the eddies, or vortices,—the "deadwater" so to speak behind the front surface—did have some effect on the resultant action. But in the present state of our knowledge such effect was largely conjectural, and to be deduced by experiment as a percentage of the calculable action on the front surface. Further, there were two experiments—to wit, of Dr. Thomas Young, and Prof. Willis—which seemed to have some bearing on the matter; and even if they did not throw much *light* on the subject, should be mentioned. Mr. Middleton then alluded to Dr. Young's curious discovery that by ejecting a current of air against a bent and suspended metal plate, the latter moved forward to the jet. (This experiment was repeated successfully in 1890 before the A.S.G.B. But the printer drew the figure wrongly in the A.S.G.B. report for said year.) However, it was very difficult to say what was the effect of the current on the recurved portion of the plate, or on the part to the right of the supporting string. Possibly, in *the experiment mentioned*, the effect of the vortices disturbing the statical pressures was even greater than the simpler effect caused by fluid impact. But such was a mystery; and Mr. Middleton only mentioned such for Mr. Hargrave's consideration, and not because the speaker himself attributed any preponderating influence to their action; at least in the case of the curved surfaces used in bird flight; where he considered the forward motion in soaring birds, or such as sometimes is seen in flying kites, to be almost entirely produced by the *upward* trend of currents of air whose effects on the wing, or kite, were predicable from our knowledge of the ordinary laws of hydro-mechanics.

Again, later in the evening, and in reply to some objections the Chairman made to Mr. Middleton's remarks on Dr. Young's experiment, Mr. Middleton repeated he had simply mentioned the experiments as being interesting, and as possibly bearing on the matter in hand; but he did *not* think they could hope for any sort of propelling force of sufficient magnitude to help a flying machine forward by means of so-called "aspiration." Indeed everything went to show that the action was too slight to be of any use in aeronautics.

Mr. Archibald then addressed the Meeting. He said: I shall be very pleased to say what I can on the subject. I have had the pleasure of having met Mr. Hargrave in Australia, and saw him flying his own kites in Stanley Park, and I have great pleasure in welcoming him to this country, and in seeing him amongst us. (Applause). I believe that Mr. Hargrave has done as much as any man to advance the whole

science of aeronautics, and especially through the medium of his very valuable kites. I have seen his kites flying at Blue Hill, as Mr. Maxim has, and also in conjunction with other kites flying there. They always told me that they trusted Mr. Hargrave's kite when they could not trust the others, and I was quite convinced of that, because most of the others broke up on various occasions. In fact they had a shed there devoted to the pieces of the others. (Laughter). With regard to this question of soaring machines, it seems to me that we shall, in the future, advance more in this direction than in any other. In the first place, the great point seems to be the curve, which imitates the curve of the bird's wing, and it must be admitted that birds accomplish things which we cannot do—there is no doubt about that. The Creator designed them in some way to do things which we cannot even work out by our equations. On looking at the matter, it seems to me that we might resolve this question into a simpler form if we regarded the back part of the curve as the part that does the lifting, and the front part, if there is any pressure of air there, must tend to send it forward. In that way the combination of the two surfaces might produce the effect. Of course this is only a hint on my part. If the matter were referred to some of our most profound mathematicians, it might be an advantage because it would show what was possible. The facts which Mr. Hargrave can verify show that something is done which we cannot account for. Something is done of which we cannot get the full explanation, but that does not take away from the fact that it is there, and I do not see any practical reason why it should not be developed into a means of using it as a flying-machine accessary. I regard the whole of this as preliminary, so far as flying is concerned. A flying-machine must have power behind it to drive it forward: therefore, we must have an engine of some description. I understand that Mr. Hargrave intended to fly his machine before the end of the century, but I am afraid we shall have to wait a little longer before that is accomplished. At the same time, I am one of those who believe it will be done, and I am sure, with Mr. Maxim and Mr. Hargrave working at it, that we ought to accomplish it in time. I do not know that I have anything further to say, except that the kite here is a little different to the one I remember seeing on a former occasion, and I suppose that the curved surfaces are a new feature in it. It seems to me that this curved surface is one of the elements which will probably lead to more than the flat surface. It is the development of the curves which has led to such remarkable results. I should like to see one of those new kites flying vertically. When I flew my kites in 1884 I thought it good enough if I got an angle of 36 degrees. Theoretically, according to Marvin, the angle of a plane kite seems to be about 60, though perhaps Captain Baden-Powell will tell me that he has got to a higher angle with his plane kite. With the curved kite it seems possible to get verticality. If this proves to be the case, we shall then be able to send up our instruments, with very much less rope than at present. Perhaps Mr. Hargrave will give us his opinion as to this. I hope he will go on with his experiments, and and I am sure we are all very well pleased to have him here in our midst. I hope he will not go back to Australia before he has made further improvements in his machine, and shown us what it can do. (Applause.)

The Chairman said he did not wish Mr. Hargrave to leave them that night without having made it clear to all present what had been the main points he had brought out in the experiments.

Mr. Hargrave, by the aid of figures which he drew, then explained to the Meeting how his experiments had been carried out on the sea-shore. Also the effects of light and heavy winds on the direction taken by the model.

Mr. Maxim again addressed the Meeting, giving them some illustrations from his paper on "How Birds Soar." He had seen eagles in Spain who, with their wings perfectly rigid, had gone completely across the plain, without a movement of their wings. They all knew how sea-gulls rested, as it were, on the air. He had seen eagles in the Pyrenees which with extended wings had balanced themselves on the ascending breeze. He said that most of the audience had, at some time, noticed how gulls followed a ship. Those birds were easy enough in a certain position, but the moment they got out of that position they had to work with their wings. As a matter of fact, they were falling as related to the wind. Every body that they saw soaring in the air was really falling so far as related to the air. He also mentioned the glassy streaks seen in the Mediterranean Sea and elsewhere, which he submitted were perhaps in part due to the currents of air meeting and ascending and coming down again at a distance. Some birds had most sensitive wings: they could feel the exact pressure and direction of the air, and were able to choose and keep their position with a nicety. The direction of the wind was also due to the earth's formation.

The wind generally moved parallel with the surface over which he blew. Mr. Maxim also told the story of a sailing ship at Erith. It was always found handy to have a tug to tow the ship into port, so the engines of the tug were put into the ship, when it was thought that they would have the same power over the ship as when they were in the tug, but which ever way the engines turned, the ship would back. (Laughter.) Mr. Maxim also described the experiments he had made with the screw propeller. He had made a propeller that would back no matter which way it was run As he had said before, the air was constantly changing, and birds were able to soar as they did by taking advantage of ascending currents

The Chairman said that Mr. Hargrave had told them the conditions under which he obtained what he believed to be evidence of this wooden model aspiring or going to windward, and he thought Mr. Hargrave put the fact down to some such phenomena as Mr. Maxim had described, to some alternations and twistings of the wind, of which the machine was able to take advantage.

Mr. Hargrave said that no one believed now that the wind blew straight, and the machine doubtless took advantage of the currents. An up-trend in the wind would account for anything. He then read a short extract from a paper as to the soaring wing of a bird.

The Chairman said that he could not agree with what Mr. Middleton had told them about the bent metal plate moving to windward against a forced blast of air. His explanation seemed to imply perpetual motion, or the getting power out of nothing. He thought that aspiration from such an action as Mr. Middleton had described was impossible.

Captain Baden-Powell (who was cordially received) said: As a great deal has been said about the ascending currents of air, I may perhaps be allowed to say a word or two about the experiments which I have made with kites which certainly impressed me with the fact that there are nearly always ascending currents if there is any wind blowing. There is one experiment I made that almost alarmed me. I was flying one of my kites which had got to a very high angle, and it steadily rose until it got absolutely over head, and the string was hanging perpendicularly. I thought that this was an extraordinary result, so I took up a heavy piece of wood, and I tied it to the string, and it steadily rose. I thought I had come across some marvellous discovery, and I should think it remained suspended for perhaps twenty seconds, hovering in mid air without any connection with the ground, and then the kite began to drift off to leeward, and the wood came to the ground, and I then arrived at the conclusion that this was solely owing to the wind taking temporarily a strong up-turn. We were also speaking of the kites at Blue Hill rising to a very high angle when being pulled down. I have continually found with kites that, if you draw in the string very rapidly, they will pass absolutely overhead, in fact, I could always, if I wanted, by drawing in my kite rapidly, work it up to windward, so that it fell to the ground to the windward of where the string was being held. I have shown my kites once or twice to the members, so that they all know their form. They are very different from the carefully constructed models we see here. Mine are certainly not so scientifically constructed, though they may be more easy to handle or to fold up or open. I have not had the advantage of flying any of these carefully-made kites, and so I should not like to say that mine are in any way better. I think that we all thoroughly appreciate Mr. Hargrave's visit here to-night. (Applause.) We know that he has not only experimented in these kites which we see before us, and in this very interesting soaring model which we really can none of us understand, but we know also that he has made a great number of experiments in other directions. We know that he has made several most interesting models of engines, I think in steam, oil, compressed air and other varieties. I am sure, therefore, that whether Mr. Hargrave is successful or not in producing a flying-machine, at any rate, his name will be always remembered in connection with the subject. (Applause.)

Mr. Davidson asked: What is the practical value of the discussion this evening. We want something practical—are we getting it?

The Chairman: The subject of the discussion is really more than anything, this. Mr. Hargrave has shown us his kites, and has given us evidence to believe that aspiration is possible under certain circumstances, and we want to come to the conclusion whether it is, or whether it is not practical.

Mr. Davidson: Whether it is, or is not; whether there is an upward tend or whether there is not, if we are going to have a practical machine we ought not to rely upon the uncertainty of these upward tends in the air. If we have to rely upon these, what is the value of the machine. We cannot always get them. Mr. Maxim has pointed out that a bird is easy in one position but not in another, and has to choose its position—we cannot do that. If we are going to make any machine that is really going to travel through the air, we must

not rely upon these things, and it seems to me to be entirely beyond the question to go into, and endeavour to find out about them.

Mr. Maxim said he thought they might compare soaring birds to the old sailing ships with two sails. They did not travel by those at the present time, they used steamships. There were several classes of birds which he might compare to a steamship or a locomotive, where a vast amount of energy was expended. The birds that followed a ship probably did not exert any power at all. Geese, ducks, partridges and similar birds did not take the wind into consideration at all: they simply forged ahead, independent of which way the wind was blowing, at the expense of a large amount of power. These exerted sufficient power to be independent of the wind—that was what we must do. Then came the question of getting machines of the right shape and form. He had spent £20,000 in that direction, and now a further sum was wanted, and he was saving up—walking instead of taking cabs, etc. to save—(Laughter)—another £20,000. He did not expect they would succeed all at once; he thought it was a matter of time.

Mr. Alfred Walker said that aspiration was spoken of in two senses, a Frenchman (Mouillard) had published a book giving his observations of the soaring of vultures in Egypt, and expressing the hope of Manflying in still air without expenditure of energy. To those of us who believe in the conservation of energy, aspiration in this sense is impossible; but if by aspiration we mean flying in a wind without expenditure of energy, this might be possible by getting energy from the eddies in the wind. The wave boat invented some years since by Mr. Hargrave is a case of aspiration in water. With respect to the motion of the S shaped surface, it seemed to be akin to the motion of the ball which goes towards the jet in a ball hydrant, and they could not hope for anything from that because the jet would have to be moved as the ball moved. He had experimented with a somewhat similar model to the one in question, which he had tied to a fishing rod. He had a small horizontal surface for the tail, and when he had tried to do without that horizontal surface, the machine had a great tendency to duck down at the front. He should like to ask Mr. Hargrave whether he had tried this model as a kite, and whether there was any similar tendency to duck down at the front. He also made other enquires as to Mr. Hargrave's experiments. Mr. Maxim had given them the result of his very interesting observations, and had explained soaring on the theory of an upward current; this, no doubt, applied to many cases of soaring, but not to all, perhaps not to the case of the albatross. Lilienthal had spoken of a wind blowing over level ground with an average tendency upwards of 3°, this would appear impossible as it would at once result in a vacuum at the surface of the ground. The most important point to be noted in flying a kite is the angle of the string, or how near the zenith will the kite remain in steady flight over level ground, neither his (Mr. Walker's) own experiments, nor any records he had seen showed this to be less than 30° away from the zenith in the case of box kites, and until a far better angle than this is attained we canot hope to fly.

Dr. Biddle thought that not quite enough had been made of the law of action and reaction. Being curved downward at the back, as the horizontal wind came, it deflected the plane of wind upwards, and the reaction tended to have a forward effect. They had more of the surface curved at the back than in front; the consequence was that the tendency would be to go forward as it rose, being pressed by the resistance of air to its rising. At any rate, that might have some influence. His theory of the matter was based to a large extent on the well-known laws of action and reaction.

Captain Baden-Powell said that, in reference to the remarks made by Mr. Walker as to the curved model being flown as a kite, he had made just such a model some years ago and tried it, and had had the same results—that it would always duck forward. As to what the speaker had said about kites flying at no better angle than thirty, he might say that although the angles of the strings of his kites had varied a great deal, according to the weather and other circumstances, yet he could always get an angle of at least forty-five.

Mr. Walker explained that he meant thirty degrees from the vertical.

Mr. Reid said that doubtless many members would like to try experiments with the Hargrave kites, and would be glad to learn where they were procurable.

The Chairman: I think I had better state at once that Mr. Hargrave has most kindly given the whole of the kites to Captain Baden-Powell, and he, in turn, has made them a present to the Society, and they will be available, some day or other, for anybody to come and look at and measure up—I believe even to practise with. (Applause.)

Mr. Reid asked if Mr. Hargrave had tried celluloid in his inventions. With regard to

ascending currents, he thought that the spot chosen by Mr. Hargrave—the sea-shore—was precisely the spot where an ascending current would be found, but you could not have ascending currents everywhere. He should like to ask whether Mr. Hargrave took the directions of the air.

Mr. Hare said he should like to ask Mr. Hargrave whether he had tried the effect of suspending his curved kite, not by one string from the middle, but by a string fore and aft.

Mr. Hargrave in answering the questions which had been put said that he thought it best to pass over any remarks that had been made about an ascending tend in the wind, because in an ascending current, almost anything could be done. With regard to the angle at which a kite could float, if they hauled any kite down, it would come down at a zenith. He believed that was common to any kite. He said he had never flown one of his wood model machines with string, as an ordinary kite. He had never used celluloid himself for his machine. He had used vulcanite, but had not found it good; it buckled and twisted, and was not so stiff as wood.

The Chairman: I think we have all to thank Mr. Hargrave very much for his most interesting paper which he had given them under the auspices of the Society. He was sure they would fully appreciate the great trouble he had taken in bringing all those beautiful kites to show them. (Applause.)

He thought everybody would admire the beauty and the details of design of the kites. Big kites were generally such exceedingly clumsy things: they wou'd not fold up, and needed several men to handle them, whereas Mr. Hargrave's kites could easily be managed single handed. Mr. Hargrave has remarked that he has sown seeds which he does not expect will bear any immediate fruit, but let us hope that Mr. Hargrave's seeds will not lie dormant, but let us try as a body, backed by Captain Baden-Powell's great energy to do our best to let these seeds bear immediate fruit. (Cheers.) I think the best thing that can be done, since Mr. Hargrave has given this collection of kites to the Society, will be for the Society to try and subscribe enough funds to found a club-house somewhere out of London at which experiments can be made, and where the Society can store any flying apparatus (which is occasionally rather a white elephant) or anything else that may be made, and where members can go and try experiments with the models and things which others have made. I believe you will hear further from the Secretary as to this. I just wish to say that Dr. Barton was to have read a paper to-night on "Navigable Balloons," but he has consented to postpone his paper to another time, as it is now so late. When we have passed a vote of thanks to Mr. Hargrave, Dr. Biddle will explain the piece of apparatus that he has brought with him as to the steering of balloons. You will remember what was said on this subject in the last number of the Aeronautical Journal. I now call upon the Meeting for a vote of thanks to Mr. Hargrave.

Everybody present heartily responded to the Chairman's request, and gave Mr. Hargrave a most enthusiastic ovation.

In acknowledging the vote, Mr. Hargrave said that his task had been a labour of love. He felt he knew a few things concerning the matter, and he hoped to forage out more.

Dr. Biddle's Apparatus.

Dr. Biddle then gave a short description of his balloon-steering apparatus* to the Meeting, saying that he had brought the sail to show to them at the suggestion of Captain Baden-Powell. The idea was exceedingly simple: it had been mooted years ago by a Frenchman, but had not been carried out in the same manner, as he had sails acting on the same principle attached to the bag of the balloon, whereas his (Dr. Biddle's) apparatus was intended to be placed between the car and the bag. This he contended was the best position. Dr. Biddle also explained the rudder, which, he said, was constructed on much the same principle as the mainsail. The apparatus was only effective during the ascent and descent of the balloon. It was dependent for its action upon the resistance of the air. The contrivance had been exhibited to the Balloon Committee of the War Office 25 years ago, and he had also read a paper on the subject (which paper had been reprinted) to a meeting of the Balloon Society of Great Britain. It had also been exhibited at the Royal Aquarium. The War Office were not satisfied with anything that did not enable them to steer a balloon horizontally. He (Dr. Biddle) would have liked to have exhibited to that Meeting, could he have done so with safety, the apparatus as applied to a balloon. (Applause.)

* Described in the last (April) number of the "Aeronautical Journal."

On Flapping Flight of Aeroplanes.

By MAURICE F. FITZ-GERALD, Professor of Engineering, Queen's College, Belfast.

[From the Proceedings of the Royal Society.]

It has been long known, principally through experiments on soaring, that a large, if not by far the largest, part of the supporting force obtained by birds in regular flight, is probably furnished by upward air pressure on their wings, regarded as planes moving horizontally, with their surfaces slightly inclined to the direction of motion.

Langley's "Experiments on Aero-dynamics" furnish some numerical data for estimating the power required to sustain an aeroplane of given weight, propelled horizontally by a known force, and he applies these to the determination of the problem whether this could be effected by screw propellers, analogous to those of a ship, actuated by machinery of existing type.

The older mathematical investigation by Navier of the problem of flapping flight, seems to be quite discredited, and, indeed, the results brought out always laboured under the physiological difficulty of demanding an amount of horse power per pound of muscle in birds which surpassed, to an almost incredible extent, that available from the better known muscular tissues of other animals. One horse power is 550 ft.-lbs. per second; a horse can ordinarily exert about two-thirds of this, and if we take him to weigh about 10 cwt., we get about a third of a ft.-lb. per second per pound of horse. A man can work at the rate of about 50 ft.-lbs. per second; this, again, is about a third of a ft.-lb. per second per pound of man; and so on with other animals. But, for the flight of birds, it was made out that from about 30 to 1300 ft.-lbs. per second per pound of bird were necessary, which, even after all allowance for higher temperature of their bodies, and large relative mass of wing muscles, compared with those of the limbs of horses, men, &c., seemed highly improbable.

In the following paper an attempt is made to indicate how both progressive and hovering flight may be effected by aeroplanes, attached to a heavy mass, and flapped after the manner of wings, under conditions sufficiently nearly approaching those of Langley's experiments to justify the inference, from his figures, of numerical results, not indeed presumably exact, but sufficiently indicating the order of magnitude of the quantities involved. The figures given may be taken, commercially speaking, as near enough to the truth to enable us to judge whether we are dealing with pounds or with pence, though not exact enough to adjust the change out of half-a-crown in paying for our power.

Let, then, a heavy mass be supposed to be flying through the air horizontally, and be supported by wings, consisting of planes of negligible mass, held nearly edgewise to the direction of motion, and moved vertically up and down by some machinery carried by the heavy mass. Let there be in addition some arrangement of the machinery by which the angle of inclination of the planes of wings to their direction of motion is variable. We shall suppose the velocity of progression high, and variations of the propelling force small enough for changes of forward velocity to be neglected, compared to the average forward velocity. This is justified by Langley's experiments, which show that, for small inclinations of an aeroplane, the direct resistance to forward motion is small compared to the supporting force. We shall also, in the first instance, neglect direct air resistance on the heavy mass as small compared with that on the aeroplanes; and, for convenience, we shall call the heavy mass the bird's body and the attached aeroplane the wings.

The case differs from that of a real bird most notably by the circumstances that the aeroplane in our theoretical case has no mass, whereas, a bird's wings have one of very sensible magnitude compared with that of its body, and the aeroplane is supposed to be moved up and down, relatively to the mass, as a whole, instead of being pivoted to it, as a birds wings are to its body, besides being of constant area, which wings are not. Consequently, as before remarked, numerical results can only be regarded as indicating the order of magnitude of the quantities calculated, not their exact values.

The inclination of the wings being variable, and their motion being compounded of an up-and-down one with a forward one, it is evident that the supporting force may be periodically variable also, and consequently the bird's body will move in a sinuous or wavy path, on the whole horizontal, and the stroke of the wings will be the relative motion of wings and body. If the horizontal velocity be high, and the amplitude of the wing stroke relatively to the air moderate, and the variations

in the supporting force not too great, the path of the bird's body will be only slightly waved so that fig. 1 may be taken as, in a general sort of way, representing what takes place.

In the same figure A_1B_1, A_2B_2, &c., represent the plane of the wing at different successive positions, seen edgewise, and P_1, P_2, &c., the corresponding resultant air pressures on it, in direction and magnitude. Now it is plainly a matter of arrangement of mechanism what these shall be, as they depend on the angle between the lines A_1B_1, &c., and the tangent to the heavy line, marked "Path of wing" at each instant. Consequently it is worth while inquiring what are the conditions of adjustment to cause a forward force on an average to be applied to the bird and wings, when the wings are moved up and down by an engine forming part of the bird's body, when the supporting force is, on an average, equal to the bird's weight, and the forward force is, on an average, equal to that required, on an average, to propel the aeroplane at its average inclination. Observe that the force P_n depends for its magnitude, not on the actual inclination of the wing path at all, but on the angle between that path and the plane of the wing, while its horizontal component depends on both angles, so that although, as long as there is any supporting force, there is a resistance to forward motion along the wing path, there may nevertheless be a forward force acting horizontally on the whole machine, wings and body taken together, as in the first position shown, for instance.

The thing to do, then, is to formulate this in symbols, and for the present purpose it will suffice to make some simplifying approximations to facilitate the work. We shall then take it that the inclination of the wing path to the horizontal, and the inclination of the plane of the wing to the wing path, are both small enough to assume that for either of them, as well as their sum, the circular measure, the sine, and the tangent do not sensibly differ, and the cosine is unity, for example, for 20° circular measure = 0·349, sine = 0·342, tangent = 0·363, and cosine = 0·94; so that up to this at any rate, we shall not be incurring errors exceeding 5 or 6 per cent. by this assumption.

We shall also assume that for a small angle (α) of inclination of an aeroplane to its path, the resultant air pressure is given by the formu a $P\alpha = 2kV^2 \sin \alpha$ in pounds per square foot, where V is velocity in feet per second, and k a coefficient, which, according to Langley, is about 0·0017, and $P\alpha$ is directed normally to the plane. This agrees nearly with Du Chemins's formula at small angles, as pointed out by Langley.

Now let the angle α be varied according to the law

$$\alpha = m(1 - \mu \cos pt),$$

m and μ being arbitrary constants, p being $2\pi f$ where f is the frequency or number of flaps per second, t being time in seconds.

Then the normal pressure, $P\alpha$, on a plane of area A, at forward velocity V, inclined at α to its direction of motion is

$$P\alpha = 2kV^2Am(1 - \mu \cos pt).$$

For convenience we shall take the bird's weight as 1 lb., so that, in general, $P\alpha$ will be for any other weight, normal pressure in lbs. per lb. of bird, and A is wing area in square feet per lb. of bird.

Let, at any moment, Z be the height above some datum level of the bird's centre of gravity, and z that of centre of pressure of wings and suppose that

$$z = S \sin (pt + \theta).$$

Then the slope, s, of the wing path is at any moment sensibly

$$s = \frac{pS}{V}(pt + \theta)$$

supposed to be always a small angle.

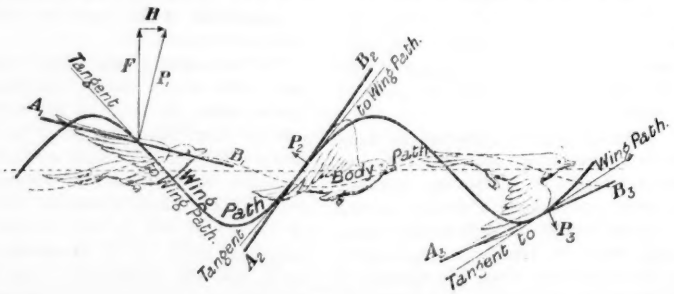


FIG 1.

By fig. 1 we see that if F be the vertical supporting force,

$$F = P\alpha \cos (s + \alpha) = P\alpha \text{ sensibly,}$$

since $(s + \alpha)$ is supposed a small angle ; and if H be the horizontal component of $P\alpha$,

$$H = - P\alpha \sin (s + \alpha)$$
$$= - P\alpha(s + \alpha) \text{ nearly.}$$

This is the resistance to forward motion horizontally, and the work done against it, taking the average rate of working, is

$$U = \frac{p}{2\pi}\int_0^{2\pi/p} VP\alpha(s + \alpha)dt \text{ ft.-lbs. per second.}$$

Again, if the bird's body be rising at the rate dZ/dt, and the wings at a lesser rate, dz/dt, the engine attached to the body, which exerts a downward force, F, on the wings, is doing work, so that, when F is positive, and $\frac{d}{dt}(Z - z)$ positive, the engine does work, and this is to be the same work, on the average, as that done to overcome the resistance of the air to the plane taken along its actual path, and is

$$W = \frac{p}{2\pi}\int_0^{2\pi/p} P\alpha \frac{d}{dt}(Z - z)dt.$$

Consequently we get, as a condition for determining some relations among the constants,

$$\int_0^{2\pi/p} VP\alpha ds = \int_0^{2\pi/p} P\alpha \frac{d}{dt}(Z - z)dt \text{.....(A)}$$

Another condition arises as follows. As F, the vertical force, is sensibly equal to $P\alpha$, we have, for the motion of the bird's body,

$$\frac{d^2Z}{dt^2} = g(P\alpha - 1), \text{ the bird being 1 lb. weight,}$$
$$= g\{2k\alpha V^2 m(1 - \mu \cos pt) - 1\}$$

and, on integration, all terms containing t^2 or t as factors must vanish, in order for the motion to be, on an average, horizontal. That containing t^2 as a factor will not vanish unless

$$2k\alpha V^2 m = 1 \text{ which fixes } m, \text{..... (B)}$$

and gives $\frac{dZ}{dt} = -\frac{\mu g}{p}\sin pt$ as there is no term with t as factor in Z.

By differentiation

$$\frac{dz}{dt} = pS \cos (pt + \theta),$$

whence

$$\frac{d}{dt}(Z - z) = -\left\{\frac{\mu g}{p}\sin pt + pS \cos (pt + \theta)\right\}$$

Inserting their values in (A) for the quantities involved, we get

$$\int_0^{2\pi/p} \frac{V}{2k\alpha V^2}(1 - \mu \cos pt)^2 dt = -\int_0^{2\pi/p} (1 - \mu \cos pt)\left[\frac{\mu g}{p}\sin pt + pS \cos (pt + \theta)\right] dt$$

On integration, the only terms that do not vanish give the equation

$$\frac{1}{2k\alpha V} + \tfrac{1}{2} \cdot \frac{\mu^2}{2k\alpha V} = \tfrac{1}{2}\mu pS \cos \theta,$$

which is quadratic for μ, namely,

$$\mu^2 - 2k\alpha V pS \cos \theta \mu + 2 = 0 \text{ .....(C)}$$

Taking the integral of one side alone we find

$$W = \frac{\mu pS \cos \theta}{2} \text{ .....(D)}$$

Eliminating μ from these we get

$$pS \cos \theta = \pm \frac{W}{\sqrt{(k\alpha V W - \frac{1}{2})}} \text{ .....(E)}$$

If now we draw a curve whose ordinate, y, is $pS \cos \theta$, and abscissa, x, is W, we find it to be of the form shown as AB or CD in fig. 1, with a symmetrical branch below the axis of x, not shown in the figure. On account of the steepness of the first part of the curve CD, that part of it which extends from W = 2·5 to W = 3 is shown with the scale of abscissæ magnified fifty times, as AB, and corresponding curves for μ are given. The case taken is when $\alpha V = 120$, so that it applies to flight at 60 feet per second, if the wing area is 2 square feet per pound carried, 30 feet per second with an allowance of 4 square feet per pound, and so on.

The principal things to note about the curves are that, in the first place, W has a minimum value, in this case 2·5 ft.-lbs. per second per pound carried. This occurs when $pS \cos \theta$ is infinite, and practically implies that when the rate of flapping is very high, keeping the same stroke, the horse-power required comes very near the minimum, and, remarkably enough, the wing becomes, virtually, exactly equivalent to a plane of area A, inclined at a constant angle $\alpha = m$, moving horizontally, as in Langley's experiments. This is not accidental, but seems to follow from the circumstance that $2W = \mu pS \cos \theta$, and if $pS \cos \theta$ be infinite and W finite, μ must vanish, and, consequently, $= m(1 - \mu \cos \theta)$ becomes $\alpha = m$ simply.

[*Note added March* 21, 1899.—In the paper as read an erroneous statement here followed, to the effect that the minimum value of W was half that got in Langley's experiments with a plane at angle $\alpha = m$. It arose from the left-hand side in equation (A) being, by mistake, written $\int_0^{2\pi/p} VP\alpha(s + \alpha)dt$, instead of as given above. The author is indebted to Prof. J. Purser for pointing out the mistake. The least value of W is equal to that of Langley's experiments.]

Of course, as before mentioned, it is not to be taken that the figure 2·65 ft.-lbs. per second

per pound of bird, is here demonstrated to be that which a real bird, with 2 square feet of wing per lb. of bird, flying at 60 feet per second, would or must exert. All that is professed to be shown is, that the bird may be able to fly with the exertion of two or three ft.-lbs. per second per pound, and that 30 to 1300 are quite unnecessary. Further, it appears that, within a very large range of the values of pS cos θ, that is frequency of flapping, stroke, great range of variation in the manner of birds' use of their wings; some (as pigeons and ducks) flapping very fast, others (as seagulls and herons) flapping but slowly, without any physiological reason for expecting material differences in the rate at which their muscles can give out energy. There are, no doubt, plenty of other reasons, but until experiments can be made on planes or other surfaces flapped by pivoting on an axis, numerical data for

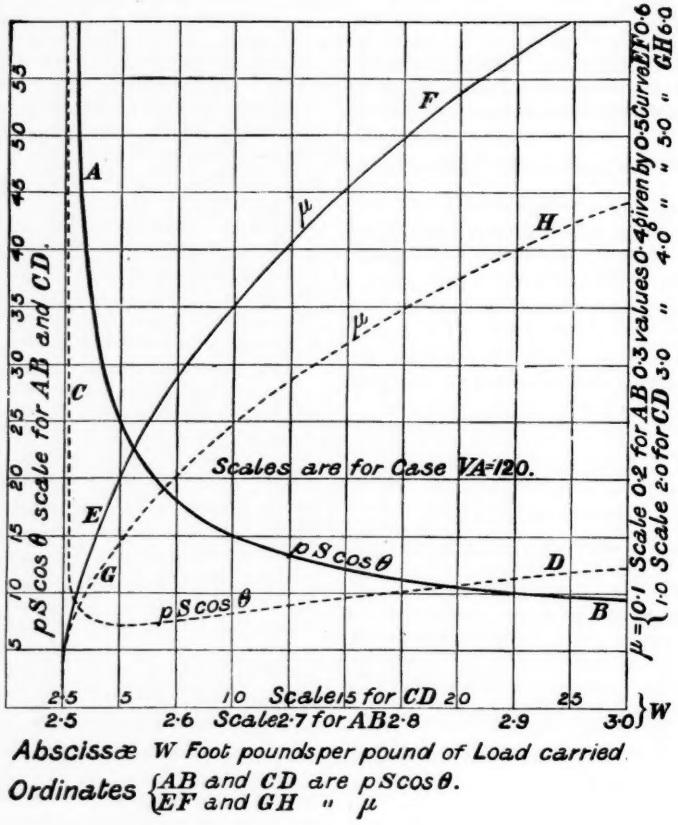

pS cos θ scale for AB and CD.
Scales are for Case VA=120.
A
B
C
D
E
F
G
H
μ
pS cos θ
pS cos θ
μ=10·1 Scale 0·2 for AB 0·3 values 0·4 given by 0·5 Curve EF 0·6 GH 6·0
1·0 Scale 2·0 for CD 3·0 " 4·0 " 5·0 " 6·0
Scale 1·5 for CD 2·0
Scale 2·7 for AB 2·8
W
Abscissæ W Foot pounds per pound of Load carried
Ordinates {AB and CD are pS cos θ.
EF and GH " μ

FIG. 2.

and lag of phase of stroke relatively to phase of bird's body motion and twisting of wing, there may be little comparative variation in the horse-power—*e.g.*, between frequency per minute 500 and frequency 250 (supposing S cos $\theta = 1$), there would only be a change of work per second from 2·51 to 2·55—scarcely 2 per cent.

This seems to bear on the explanation of the conditions of calculation agreeing more nearly than those here used with conditions of fact, seem to be wanting.

Another point brought out is that pS cos θ has a minimum value as well as W, but μ has not. This means that the phase of the wing motion, relatively to the twisting of the wing (on which the angle α depends) is important, and if these are out of beat, so to speak, by

more than a certain amount, with a given frequency and stroke, the flight cannot be maintained at all. The values of μ which exceed unity imply (on reference to the equation $a = m(1 - \mu \cos pt)$) that there is a reversed pressure on the wing during part of the stroke, as shown in fig. 1 at the third position, where P_3 is directed partly downwards. This condition is however unfavourable, probably in all cases, to economy of labour, though it may be favourable to forward propulsion. At all economical rates of working μ is quite small, that is, the inclination of wing plane to wing path varies but little.

It may also be observed that, for any value of pS cos θ, there are two of W, one of which lies close to its least possible value, and the other is very large—*e.g.*, for pS cos $\theta = 15$, W = 2·65 or about 40, so that a person who was going on observed values of pS and took cos θ to be nearly unity, might easily overlook the small value of W, and base his estimate of work on the large one. This is the more likely as the curve of pS cos θ is of the third degree, and the solving of a cubic by a direct process, especially when there are three real roots, is troublesome, and avoided in consequence. Whether anything of this kind occurred in Navier's or other earlier investigations, the present writer is unable to say. Langley says that Navier added the work done, here expressed by the right-hand side of equation (A) to that on the other to find the whole. Why it should have seemed necessary to do this is not clear, for if the wing has no sensible mass, as here assumed, the pressure of the engine downwards cannot exceed F, and no work can be done by a vertically moving engine, except that expressed on the right-hand side of equation (A). The way in which forward motion is maintained bears some analogy to the process of so-called "invisible" skating, and to screw propulsion, wherein it would be manifestly absurd to add the work done against the ship's resistance to that given by the turning moment applied to the screw, to find the horse-power of the engine. In fact, the construction of Langley's whirling table virtually made the aeroplane a portion of a screw propeller with axis vertical, and his coefficient k has, bound up in it, the inefficiency of the propeller in converting horse-power of turning couple into thrust.

With regard to the absolute magnitude of the figure 2·5 as the least value possible for W in the case illustrated by fig. 2, it is of importance to note that this depends on m, and m has been made $= \frac{1}{2k\mathrm{AV}^2}$ on the strength of the equation

$$\frac{d^2\mathrm{Z}}{dt} = g(\mathrm{P}a - 1) = g\left\{2k\mathrm{AV}^2 m(1 - \mu \cos pt) - 1\right\}$$

which again depends on the assumption that the downward acceleration of the body attached to the aeroplane would be g, irrespective of the horizontal velocity of the plane, if the angle of inclination of the plane to its path were zero. Langley's experiments on the "Plane Dropper" seem to show that this is by no means the case, but, unfortunately, he was unable, through uncontrollable circumstances, to determine the proper value, which is some function of the velocity, and may possibly be so small as $g/3$ at his highest velocities. If so mV which is the minimum value of W requires corresponding reduction, and, in the case taken, the scale of W is about three times too large, reducing the necessary work to about 1 ft.-lb. per pound carried or thereabouts. It is now evident how the energy expenditure of birds may lie within quite reasonable limits, though we cannot, with any approach to accuracy, make the calculations for the design of a flying machine with flapping wings. We can say it will not in a given case cost a pound an hour for power, but we cannot tell whether it will cost only sixpence, or as much as half-a-crown or three shillings.

To allow for head resistance of the bird's body some term must be added to the left-hand side of equation (A), which will alter the absolute term of equation (C) and the $\frac{1}{2}$ under the square root in equation (E) so as to increase W, and a number of small corrections of other kinds might be made, which, however, considering the roughness of the numerical data we have to go upon, are not worth taking into account.

The justification of the assumption as to the smallness of a and s is best exhibited by a numerical example. Taking then AV=120 with A = 2 square feet per pound of bird, and assuming S = 0·5 (a stroke of 1 foot) we find that if $p = 30$ (about 300 downward strokes per minute, which sea-gulls in regular flight often exceed) and cos $\theta = 1$, Sp cos $\theta = 15$, and W = 2·65; while μ is 0·35, amd m is 0·041. The maximum value of a is then $0{\cdot}041(1 + 0{\cdot}35)$ when $pt = \pi$. This is 0·0554, the circular measure of 3° 10′ nearly. The maximum value of s, the slope of wing path, is pS/V = 0·25; the circular measure of 14° 20′; but these do not occur simultaneously.

It will be found that their sum is a maximum when $pt = 0$ and is then 0·28, the circular measure of 16°; thus lying well below the limit

of 20° suggested in the earlier part of this paper. The body-path on integration, and taking $Z = 0$ and $dZ/dt = 0$ initially, is $Z = -\mu g/p^2 (1 - \cos pt)$ and the amplitude is 0·0125 foot. The body, therefore, pursues a simple harmonic path, whose average is about 5/32 inch below the mean level of the wing path, and whose total rise and fall from crest to hollow is almost 5/16 inch. The relative motion of wing and body does not sensibly differ from that of the wing alone.

The investigation of the question as to hovering is far less satisfactory. We might assume, for instance, that the centre of pressure of the wings followed a circular or elliptical orbit, the plane of the wings making a small angle with the tangent to the orbit, and being variable as before, so as to maintain, on the whole, an upward pressure. If we make this angle $\alpha = m \sin pt$, and form the equation $d^2Z/dt^2 = g(F-1)$ as before, remembering, however, that though α may be assumed small, the angle of slope, s, of the wing path, is not, we shall find, if the orbit be taken to be circular, $m = 1kV^2$ where V is the velocity of the motion along the orbit. In order that m may have values similar to those in the case of progressive flight, the frequency of flapping must be much higher, twice as fast, or thereabouts. But there are several considerations which render results of this kind much more unsatisfactory than those previously obtained. In the first place the motion assumed is very much less like that of a bird's wing than in the former case, inasmuch as it involves complete revolution of the plane of the wing about a horizontal axis in its own plane, and, in the second place, the edges of the plane are continually cutting across the eddies, created by their previous motion, in a very different way from that in which they interfere with those created in progressive flight, so that the numerical value of k, and the value of the function of V which is put down as 1 in the form la $d^2Z/dt^2 = g(F-1)$ are really unknown, and cannot be inferred from experiments with soaring planes, or even propellers. We can only expect that the actual numbers will be tolerably like those given by Langley's and other such experiments.

All that can be inferred, therefore, is that, provided a sufficiently high speed of flapping is attainable, we may reasonably anticipate that the horse-power for hovering need not differ very materially from that for progressive flight. Internal stresses in the wings or machinery due to heir inertia, as well as physiological difficulties connected with high velocities of reciprocation, may also come in to limit the rate of flapping, and prevent an equally economical rate of working being attainable in hovering, as in progressive flight. A considerable simplification of the mere mathematics could have been effected by at once assuming that the arrangement for altering the angle α was so contrived as to make the pressure $P\alpha$, or its vertical component F, an assigned function of the time; but this has the objection that, there being then no expressly formulated relation between α and P or F, experimental evidence would be wanting to settle the values of the numerical constants involved. In fact, the more the mathematical work is generalized, the less definite do the numerical results become, in the present state of our experimental knowledge, and this must form the writer's apology for working out, in a mathematically clumsy way, so limited a case of a problem which has been treated already, in a much more powerful and general way, by far abler hands. For example, Thomson and Tait mention, as an example of motion of a solid in a liquid, the vibratory and irregular movements of such an object as an oyster shell sinking pretty slowly with a swaying motion when thrown flatwise into water. The rate of expenditure of energy necessary to prevent its sinking would, if these motions were forcibly produced, evidently practically coincide with the rate at which gravity works, when the rate of sinking is, on an average, uniform, and should be quite small. The case of hovering flight of a bird is manifestly closely analogous and that of progressive flight is similarly closely analogous to that of the same oyster shell, or a piece of slate, projected under water and forcibly maintained in a state of rhythmic motion. The only use of the investigation given in the present paper is to reduce the matter to figures, in a case sufficiently simple to enable us to use numbers supplied by existing experimental results.

[*Note added March* 21, 1899.—The error alluded to in the previous note was detected by checking equation (A) and that for H to see if the horizontal component of the acceleration was exactly periodic. Equation (C), as now given correctly, is the condition in question, with the additional information that each side is the work done. Mr. F. Purser, F.T.C.D., suggested that the wings as well as the body might be supposed heavy. The author has examined this point, and finds that, supposing, as before, that the engine, say a cylinder and piston

to whose rod the aeroplane is attached, works vertically, no difference is made in equation (C). The force Pa being inclined, has, or may have, a moment about the centre of inertia of the whole machine; the effects of this should be wholly periodic, but inasmuch as it appearsfrom experiment and otherwise that P does not pass through the centre of figure of the plane, and moves about in some way depending on the angle a, it would be impossible to test this condition without assuming details of dimensions, &c. Any actual bird or flying machine, must have some steering apparatus, capable of correcting disturbances of a rotational kind in both the vertical and the horizontal plane, but its management plainly cannot involve any materia alteration in the work to be done. The author conceives it to be possible, if not highly probable that the motion in flying is, in respect of direction, unstable. Small periodic terms may be assumed also to be added to V, but lead to nothing up to the order of quantities involved in neglecting the difference between cosine and radius.]

Some Simple Experiments with Aero-Curves.

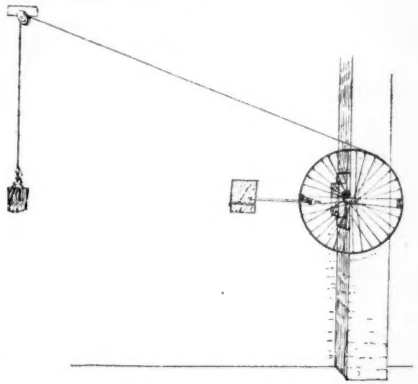
By A. A. MERRILL, of Boston, Mass., U.S.A.

In the summer of 1898 I constructed a full sized gliding machine with which I made many short flights, ranging from fifty to one hundred feet. This machine was constructed on the plans of the Herring soarer used by Mr. Octave Chanute in his experiments on the borders of Lake Michigan during the summer of 1896, and, although I was not able to make so long a flight as Mr. Herring did, I acquired some knowledge of the management of the apparatus and the difficulties to be overcome. I used the Lilienthal form of curve, which is the arc of a circle, and the height of the curve is 1/12 the length of the chord.

My experiments showed not only that the problem of stability is a very great one, but that it is very necessary to find some more efficient form of curve than the circle, in order to reduce the power necessary to drive the machine through the air at a sustaining speed. I, therefore, determined to experiment with various surfaces, until I found one offering much less resistance than the one I was using.

The question of the best methods of experimenting next arose. I found that to make quantitative experiments, measuring accurately the lift and drift in some unit of weight measurement would involve the purchase of expensive and delicate pieces of apparatus, and I therefore determined to use the qualitative method. After much discussion with other members of The Boston Aeronautical Society, I finally concluded to use a small whirling table.

The illustration shows all the working parts, and the machine is easily described. The wheel, which is taken from a bicycle, is 26″ in diameter, and its axle is fastened to a pier in

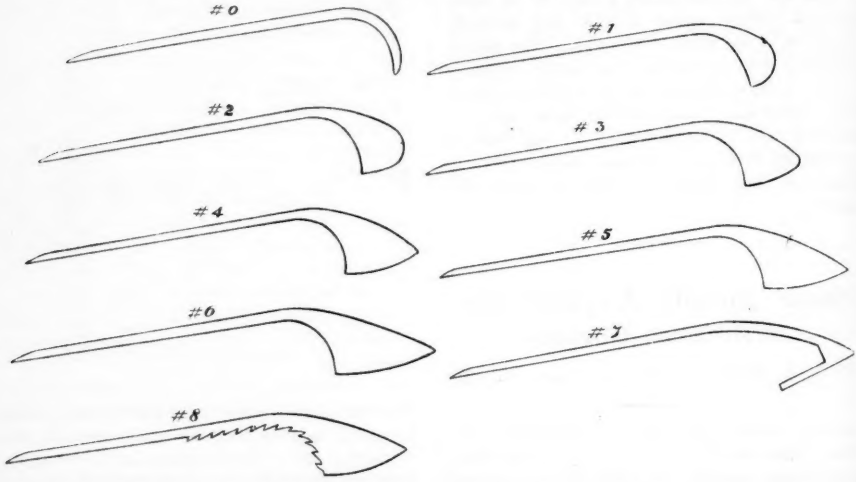


the cellar, where the experiments were made, in such a manner that the wheel revolves in a vertical plane. From a point on the periphery there is a standard of wood through the end of which a dowel is placed, and the outside end of this dowel is fastened to the surface to be tested. There is also a counterweight on the periphery to balance the wooden standard and dowel. This arrangement brings the plane of revolution of the surface parallel to and 8″ from the plane of the wheel, and the length of the dowel is such that the centre of the surface describes a circle 60″ in diameter. In the rim of the wheel is a pin over which a ring is placed. This ring is attached to a rope which goes through a pulley, and is fastened to a weight. There is also a counter which records the number of revolutions.

The mode of operation is as follows. A surface is fastened to the dowel at a known angle, the rope is attached to the wheel which is then wound up, raising the weight to the ceiling, and the reading of the counter is recorded. The wheel is then released, the weight falls, the rope leaves the wheel, and after it stops revolving a second reading is taken. By

changing the surfaces and keeping all the other factors constant, this machine enables us to determine which of any two surfaces offers the least resistance to forward motion. Much of my time was taken up in arranging the various parts in such a manner that the friction of the machine would be practically constant, and I obtained this condition after many trials, so that the greatest variation in any series of tests of the same surface was less than 2 per cent.

I next drew a series of surfaces to be tried, cross-sections of which are shown in the diagram, the surface marked 0 being the one recommended by Hargrave as being much



better than the Lilienthal surface. The other surfaces, except No. 7 and No. 8, are modifications of this curve, and differ simply in the thickness and shape of the front edge. The dimensions of the surfaces are as follows: No. 0 = 6″ × 6¼″, No. 1 = 6″ × 6½″, No. 2 = 6″ × 6¾″, No. 3 = 6″ × 7″, No. 4 = 6″ × 7¼″, No. 5 = 6″ × 7½″, No. 6 = 6″ × 7¾″. No. 8 is similar to No. 5 except for the ridges underneath, the use of which will be explained later; No. 7 differs from the rest principally in the shape of the underside. Care was taken that all these surfaces should weigh the same so that there would be no difference of weight to influence the records. All the surfaces were made of wood, and surfaces numbered 1, 2, 3, 4, 5, 6, were made by the addition of a piece of wood of the proper shape to the surface marked 0. In several series of tests of all the surfaces except No. 7 and No. 8, No. 5 was found to be the best. With No. 5 on the machine the wheel made 40 revolutions, while with No. 0 it would turn but 35 turns. This difference of 5 revolutions means a difference of 79 feet, so that with the same amount of energy expended in starting there is a gain of 14 per cent. in distance travelled. No. 2 and No. 3 gave about the same results, each revolving 32 times, while No. 4 and No. 6 revolved 39 times. It would seem, then, that to thicken the front edge and shape it properly tends to decrease the resistance, and that the limit to this thickening is reached in No. 5. In this the distance from the extreme front edge to the point where the under-surface proper begins is 20 per cent. of the whole distance to the rear edge. It may be noted in passing that in the Eddy kite, which flies at very high angles, the distance from the apex to the cross-stick is 18 per cent. of the whole distance from the top to the bottom of the kite, and I think, therefore, we may conclude that in flight the surface of this kite takes a position (by bagging) somewhat similar to the under surface of No. 5, which surface is shown by my experiments to offer much less resistance than other surfaces commonly used in kites and gliding machines. In my next experiments I modified the form of the under surface. Several people, particularly Mr. Lawrence Hargrave, of New South Wales, having advanced the theory that this surface (No. 0) caused the existence of a vortex under it which was used in soaring, I determined to see if this was true. I therefore took a small piece of writing paper to which I pasted thin strips of tissue paper in such a manner as to

overlap each other, and this paper was attached to No. 5 in the position shown in No. 8, and the surface was then tried. I was astonished to find that the addition of this small piece of paper caused the wheel to make three (3) more revolutions in every instance so long as the angle of the surface remained at 0°, and this difference was shown with every surface to which I attached the paper. This showed me that the vortex, so often spoken of by Hargrave, certainly does exist, and also that it can be made to do work in overcoming some of the resistance to forward motion, which would otherwise have to be overcome by the engine. The cause of this vortex is probably the formation of a vacuum which causes the retreating air to rush upwards and forwards to fill the empty space, thus forming a whirlwind under the surface. This whirlwind revolves in a vertical plane, so that the air which touches the surface in the curved portion is moving forward faster than the surface is, and, therefore, this air is in a condition to impart some of its energy to the surface, and this is done when the surface is roughened.

I then tried this piece of paper in many places, but the best results were obtained when it was in the position shown in No. 8, and I immediately constructed a surface having the roughness cut in the wood, and got the same results as with the piece of paper. My conclusions are that the vortex occupies the forward half of the under surface (this agrees with the results of Hargrave's experiments), and that if this portion is properly roughened the vortex may be used to advantage. No. 7 did not give good results, possibly because the vortex did not have a proper surface to form under.

Most of my experiments have been made with the surface at an angle of 0°, but I hope to carry them further and get qualitative results at other angles. I did, however, test the roughened under surface at large positive angles (such angles as kites would take), and in these cases the advantage of the roughness is less than when the surface is at 0° or —5°. The reason for this is, I believe, that at large positive angles, say 10° and more, the vortex cannot form as readily nor preserve its integrity as easily as at 0° or —5°.

This is as far as I have gone with these experiments, but they seem to show that Hargrave is right in stating that the existence of a wind vortex under a bird's wing is an important factor in soaring. I would like to have some experimenter use surface No. 8 in a large gliding machine, as I am confident that longer flights can be obtained with it than with either the Lilienthal or the simple Hargrave surface (No. 0).

I wish to say in closing that Mr. F. H. Wenham brought up the point that all birds' wings are extremely rough underneath, and in a paper read before the Boston Aeronautical Society, March 1st, 1897, put this fact forward as having a direct bearing on flight. The experiments which he was able to make at the time were not perhaps as convincing as they might have been; but in the light of Hargrave's experiments and my own work described above I am strongly inclined to think that Mr. Wenham is right, and that this roughness is important, though not in exactly the way he imagined it to be so.

I shall be very glad to hear what other experimenters have to say on this point.

NOTES.

Aeronaut's Adventure.—At Graz, in Styria, on June 26th, Signor Merighi, an Italian aeronaut, ascended in his balloon to a height of considerably over 10,000 feet, when the balloon burst suddenly and fell. At a distance of about 40 feet from the earth Signor Merighi threw himself out of the car. He broke one leg, and was taken up unconscious, but living, and was taken to a hospital. During his fall he wrote on a paper, found in the car—"Am dying contentedly, having reached my term." Signor Merighi states that at the height of 3,500 metres the balloon suddenly swelled to an enormous size, and though he immediately opened the ventilator, it burst with a tremendous crash, and was rent open from top to bottom. It formed, however, a kind of dome, which served as a parachute in the tremendous fall that followed.

Experienced French and British aeronauts, says the "Newcastle Chronicle," will be inclined to smile at the accounts of the perilous balloon ascent which has proved all but fatal to Signor Merighi. It seems plain that the adventurous Italian aeronaut might have done much better. Most balloonists up to their work would not have wasted time in scribbling dying messages on paper. They would have cut the car off, clung to the hoop, and in this way manœuvred the lightened balloon-parachute so as to land without injury whatever to themselves. Signor Merighi is to be sympathized

with, but not congratulated on his ballooning skill.

Kite Experiment on War Ship.—An experiment with kites from the bridge of the cruiser "New York" has been made to demonstrate to naval officers their value in giving signals at sea and in dropping life lines to ships in distress. Several large kites were sent up with pennant and signal flags attached. The latter were so attached to the cord that they could be lowered and changed in a short time. In making the life-line experiment a coil of cord is attached to the main kite line. By means of a small cord it was liberated when over a tug. One end was seized by tugmen and the other hauled in aboard the "New York." By means of this line a stout cable could be drawn from one boat to another.

Strange Accident to a Balloon.—An incident occurred during a recent military balloon ascent in Germany which has raised a question of extreme importance for aeronauts. Lieutenant von Sigsfeld, Engineer Tormin, and Second Lieutenant Hildebrandt, of the Berlin Military Balloon Department, ascended on Wednesday in a balloon filled with 1,300 cubic metres of coal gas and hydrogen. After eight hours' travelling the balloon was becalmed at a height of 8,000 ft. Hoping to find a current in a lower region Herr Sigsfeld pulled the cord to open the ventilator. To his astonishment it broke away in hand. On examination it was found that 2 ft. of the cord which runs up the inside of the balloon was decomposed and crumbled when touched. It is assumed that the sulphuric acid which is used in the production of the hydrogen had come into contact with the cord, perhaps through condensation. Fortunately no other lines or part of the balloon were affected, and after an anxious journey the balloon was finally landed in safety. The incident has led to a discussion of the question whether it is not better entirely to give up the use of hydrogen in aeronautics on account of the decomposing properties of sulphuric acid.—"Observer."

Naval Ballooning.—Lieut. Genty, director of the "Aerostatique" station at Toulon has been carrying out some inferesting naval balloon experiments on board the battleship Jauregiberry. The chief object of the experiments was to discover the limit of distance at which the balloon could sight an enemy's ship without betraying its own presence. At an elevation of 400 yards from the bridge of the Jauregiberry the battleship Carnot could be observed at distances varying from five to 25 miles, whilst at the longer ranges of view, the battleship could only detect the balloon by means of powerful electric reflectors. As a result of the experiments it is held as firmly established that to a man on the look-out 20 miles away, a balloon is quite invisible.

The Andree Expedition. — A Reuter telegram from Stockholm says that the Anthropological and Geographical Society in Stockholm has received the following telegram from Herr Vathne, a shipowner at Mandal:—"Captain Hueland, of the steamship 'Vaagen,' who arrived there on Monday morning, reports that when off Kola Fjord, Iceland, in 65° 34′ north lat., 21° 28′ west long., on May 14 he found a drifting buoy marked 'No. 7.' Inside the buoy was a capsule marked 'Andrée's Polar Expedition,' containing a slip of paper, on which was given the following:—'Drifting buoy No. 7. This buoy was thrown out from Andrée's balloon on July 11, 1897, 10.55 p.m., Greenwich mean time, 82° north lat., 25° east long. We are at an altitude of 600 metres; all well. Andrée, Strindberg, Fraenckel.'" Herr Andrée made his ascent from Danes Island on July 11, 1897, at 3 o'clock in the afternoon, so that when the buoy was thrown out the explorer had only travelled seven hours and 55 minutes.

The Andree Search Expedition of the Swedish Professor Nathorst will sail during the present summer, with the express encouragement of the Swedish Government, of Nansen, and of several eminent scientists and financiers. The Swedish Parliament has voted him a grant of 40,000 kroner, 50,000 kroner have been contributed by private sympathisers; and his former ship, the Antarctic, is being furnished for the expedition. Nansen asserts, in a communication to the "Allgemeine Zeitung," that he has all along been of the opinion that the balloon must have been driven in a westerly or south-westerly direction, in which case it must have made for the eastern coasts of Greenland. Here, he imagines, the balloonists may have been landed, where they would not perish for want of food, since seals, reindeer, birds, the walrus, and the bear are richly abundant. The chances of their preservation, however, are considerably lessened by the fact that the last autumn passed without any communication from them. "Nevertheless," says Nansen, "I do not think that all hope is extinct." Next month it will be two years since Andrée started upon his daring flight.

Paris Exhibition of 1900.—There will be instituted during the Exhibition a series of international contests of sports, including athletics, fencing, shooting, cycling, sailing, and ballooning. Section X. (Aerostation) will take place in the annex of the Daumesnil Lake. Some valuable prizes will be given.

Foreign Aeronautical Periodicals.

ILLUSTRIERTE AERONAUTISCHE MITTHEILUNGEN (Strassburg).

April, 1899.—Dr. Moennichs.—Ortsbestimmungen im Ballon, von Professor Dr. S. Finsterwalder.—Conditions of success in the design of flying machines (Die Bedingungen des Erfolges im Entwurf von Flugapparaten), von O. Chanute, übersetzt von Rittmeister Warder.—Steilstehende Drachen, von Oberingenieur Arnold Samuelson.—Der Drachenballon der Jubiläumsausstellung in Wien, von August Riedinger.—Ballons pour rire, par M. Georges Bans.—Zur Theorie der Luftschifffahrt und Flugtechnik, von V. Turin, Privatdocent.—Petite expérience de Parachute dirigeable, par M. le Comte Jules Carelli.—Kleinere Mittheilungen: Ein neuer Drache Aëroplane, von Chas. Zimmermann. — Andrée-Nachrichten, von Carl Christensen.—Dr. Danilewsky's Versuche in Kiew.—Die beabsichtigte Verstaatlichung der meteorologischen Luftschiffahrt in Preussen.—Fahrten über den Kanal von Frankreich nach England.—Vereins-Mittheilungen.—Aus unseren Vereinen. — Aus anderen Vereinen.—Patente in der Luftschiffahrt. — Eingegangene Bücher und Separatdrucke.—Zeitschriften-Rundschau.—Allgemeine deutsche Sportausstellung München 1899. — Aus anderen Zeitschriften. — Neue Nachrichten.

ZEITSCHRIFT FUR LUFTSCHIFFAHRT (Berlin).

Heft 2. 1899. Februar.—v. Loessl: Der aërodynamische Schwebezustand einer dünnen Platte und deren Sinkgeschwindigkeit nach der Formel $V = \sqrt{\frac{g\,G}{\gamma\,(F+bv)}}$. (Fortsetzung.) Steffen: Zur Spannungs-Theorie.—Dienstbach: Ueber Luftwiderstand. — Kleinere Mittheilungen: Dienstbach über die unzutrefende Beurtheilung der Arbeiten von Maxim und Kress durch Koch und Lorenz. — Buttenstedt: Bemerkungen zu Platte's Idee. Weisse, Bemerkungen zum Buttenstedt'schen Flugprinzip.

Heft 3. 1899. März.—Trabert: Was erwartet die Meteorologie vom Registrir-Drachen? — v. Loessl: Der aerodynamische Schwebezustand einer dunnen Platte und deren Sinkgeschwindigkeit nach der Formel $V = \sqrt{\frac{g\,G}{\gamma\,(F'+bv)}}$ (Fortsetzung.) — Kleinere Mittheilungen: Platte: Ueber das Fliegen. — Algemeine deutsche Sport-Ausstellung München. — Ueber die Bildung des Aéro-Clubs in Paris. Bücherschau: W. A. Tjurin: Zur Theorie der Luftschiffahrt.

Heft 4. 1899. April.—Dienstbach: A. M. Herring's neue Flugversuche. — Joseph Popper: Flugtechnische Studien. —v. Loessl: Der aerodynamische Schwebezustand einer dunnen Platte und deren Sinkgeschwindigkeit nach der Formel $V = \sqrt{\frac{g\,G}{\gamma\,(F+bv)}}$. (Schluss.) —Kleinere Mittheilungen: Dienstbach: Zur Entgegnung Buttenstedt's.—Platte: Ueber die Nothwendigkeit, das spezifische Gewicht der künstlichen Flugkörper jenen der Vögel mindestens gleich zu machen.

Heft 5. 1899. Mai. — Hinterstoisser: Einige Daten über die simultanen Ballonfahrten am 24. Marz 1899 in Wein. — Josef Popper: Flugtechnische Studien. — K. J.: Die Flugtechniker und die Mechanik. — Kleinere Mittheilungen: Dr. K. Danielewsky: Vergleichende Tabelle der Werthabschatzungen bei praktischer Anwendung eines Luftschiffes jetzigen Typus' und eines Flugapparates des Dr. Danielewsky. — Emil Jacob: Berichtigung. — Neue Schriften. — Vereinsnachrichten. Deutsche Verein zur Förderung der Luftschiffahrt zur Berlin. Protokolle der Versammlungen vom 27. Februar und 26. April 1899.

L'AERONAUTE (Paris).

Avril, 1899. No. 4.—Société française de navigation aérienne, séance des 6 et 20 avril 1899.—Notes sur l'enlèvement d'un cerf-volant par un temps calme au moyen d'une voiture automobile et sur les expériences de Blue Hill par M. Wentz.—Faits divers. — Ascensions internationales.—Expériences de ballons satellites par M. Mallet.—Nécrologie. M. Séraphin Magloire Jaubert. — Appareil pour la production de l'hydrogène, traduction de M. O. Frion.

Mai, 1899. No. 5.—Exposition universelle de 1900.—Liste des Comités d'installation de l'Exposition contemporaine et de l'Exposition centennale d'aéranautique. Liste des exposants provisoirement admis.—Société française de navigation aérienne, séances des 4 et 18 mai 1899.—Compte rendu par M. Lissajoux fils, de l'ascension exécutée par MM. Lissajoux père, Hioux et Poirier en 1870.

L'AEROPHILE (Paris).

Septembre-Octobre, 1898. Nos. 9-10.—Wilfrid de Fonvielle; L'Histoire de l'Aérostation scientifique: M. Cailletet (1 gravure).—G. Besançon: Expériences du Ballon dirigeable de M. de Santos—Dumont (3 grav.).—G. Besançon: Progrés de la Navigation aérienne à l'Etranger.—Gustave Hermite: Ascensions internationales du 8 juin 1898 (6 grav.).—Louis Bartalot et Léon Flameng: Impressions de voyages aériens.

Novembre-Décembre, 1899. Nos. 11-12.—Wilfrid de Fonvielle: L'Histoire de l'Aérostation scientifique: Le Général Rykatchew (1 gravure).—Edouard Surcouf: L'Ascension du Véga (6 gravures).—A. Cléry: L'Aérostation à l'Exposition Universelle.—G. Hermite: L'Ascension de l'Alliance.—Wilfrid de Fonvielle: L'Aérostation à la Société Astronomique de France.—Ch. Ducos: Ma Première Ascension.—G. Garcia: La Traversée du détroit per le ballon l'Evening News.—V. Cabalzar: La Traversée de la Manche en ballon.

Janvier, 1899. No. 1.—M. le comte de Dion (Wilfrid de Fonvielle).—Le Baptême de l'Aéroclub (Ancelle).—Un Ballon dirigeable russe (A. Cléry).—Les Gaz liquefiés et les Ballons-sondes (Wilfrid Monniot).—Les ascensions aérostatiques en Allemagne (Georges Besançon).—L'Exposition des Sports en 1900 (Cabalzar).

Février, 1899. No. 2.—Portraits d'Aéronautes contemporains: M. A.-L. Rotch (Wilfrid de Fonvielle).—Les Ballons dans la littérature (Henry de Graffigny).—Les prétendues nouvelles d'Andrée (V. Cabalzar).—Nécrologie: A. Duté Poitevin (A. Cléry).—L'Aéro-Club (G. Garcia).—Conférences sur l'Aéronautique (P. Ancelle).—Bibliographie: L'Aéronautique (A. C.).—Informations: L'Aérostation militaire étrangère; Manœuvres aérostatiques militaires à Toulon: Les Ballons a Déviateurs; Un Aéroplane à Moteur a pétrole; Voyage en Ballon; Un Legs important: Le Legs Farcot.

Mars, 1899. No. 3.—Portraits d'Aéronautes célèbres: Jules Duruof (Wilfrid de Fonvielle).—Les Cerfs-Volants et la Metéorologie (Georges Besançon).—L'Aéronautique a l'exposition de 1900 (V. Cabalzar).—Un Criterium de la Navigation aérienne (Wilfrid Monniot).—Les Ballons dans la Littérature (Henry de Graffigny).—Revue des Moteurs légers (A. Cléry).—Expérience de Direction aérienne (Gustave Hermite).—Sur un Record allemand (Paul Ancelle).

Avril, 1899. No. 4.—L'Exploration des hautes Régions de l'Atmosphère: Expériences internationales du 24 mars 1899.—Les Ballons Sondes de M. Teisserenc de Bort (Wilfrid Monniot).—Le Lancer de l'Aérophile, no 3, 24 mars 1899 (Gustave Hermite). L'Ascension du Balaschoff, 24 mars 1899 (Georges Besançon).—Rapport sur l'ascension du Balaschoff (Georges Le Cadet).—Aéro-Club.

Mai, 1899. No. 5.—Portraits d'Aéronautes contemporains: M. Abel Corot (Edouard Surcouf). Historique des divers projets d'exploration aérostatique du Pole Nord (G. L. Pesce).—La coupe des aéronautes (Georges Juchmès).—L'Aéronautique à l'exposition de 1900 (Wilfrid Monniot).

LA FRANCE AERIENNE (Paris).

No. 7. Du 1er au 15 Avril, 1899.—De l'horizontalité dans les machines aériennes, comte Jules Carelli.—Monsieur et Madame Duruof: souvenirs retrospectifs avec photographies.—Académie d'aérostation météorologique de France: Séance du 1er mars 1899.

No. 8. Du 15 au 30 Avril, 1899.—L'aéronautique à l'Exposition de 1900, avec figure: Georges Guibourg.

No. 9. Du 1er au 15 Mai 1899.—Une aéronaute oubliée: G. Guibourg.

No. 10. Du 15 au 31 Mai 1899.—Une aéronaute oubliée: Maret-Leriche.—Revue de presse.—Ballons pour rire extrait des Ill. Aëron. Mittheilungen.—Académie d'aérostation météorologique de France, séance du 19 avril 1899.

No. 11. Du 1er au 15 Juin, 1899.—Bonne foi aéronautique: Georges Guibourg.—Aéronautique au jour le jour.—Académie d'aérostation météorologique de France: séance du 3 mai 1899.—Société aérostatique des Bouches-du-Rhône: statuts.

No. 12. Du 15 au 30 Juin, 1899.—La France aérienne à Lyon.

L'AERONAUTA. Rivista mensile illustrata dell' Aeronautica e delle scienze affini.

No. 7 a 12. Giugno 1898.—Aprile 1899.—Esperienze col paracadute dirigibile del Conte Carelli-E. Vialardi. — Macchina Acrea-G. M.—Sul volo degli Uccelli-Ing. Lanzerotti. —L'Avion-E. E. Vialardi Perchè non è ancora conquistato il regno dell' aria.-Silvio Da Rin Fioretto. — L'aria liquida e la liquefazoine dei gas-E. Vialardi.—La Società Aeronautica Italiana e la Conferenza Internazionale di Aeronautica a Parigi nel 1900 - Castagneris Guido. Fra i cervi volanti-E. Vialardi. — Motore leggero Hargrave - Icaro. — Fra i Palloni dirigibili - E. Vialardi. — L'Italia, progetto di aeronave dirigibile del Cav. Ernesto De Angelis - E. Vialardi. — Conferenza del signor V. J. de Turine.—Rapporto della Commissione Internazionale Aeronautica, riunione dal 31 marzo al 4 aprile 1898 - E. Vialardi. — Notizie varie. — Fra libri e giornali.

Notable Articles.

April *McClure's Magazine*.—"The War Kite." Capt. BADEN-POWELL. (Illustrated.) Similar to, but not same as, article in January *A.J.*

6. *Naval and Military Record*.—"Ballooning in the Army." General account, illustrated by four good photographs.

22. *Revue Scientifique* (Paris).—"Les Ballons Sondes." Review of book by M. DE FONVIELLE.

23. *Sunday Daily Mail*. "Gun Cotton in the Clouds." Illustrated account of Mr. Bacon's experiments.

May *Journal des Sciences Militaire* (Paris).—"Military Ballooning."
Strand.—"The Newest Flying Machine."

14. *Sunday Daily Mail*.—"French and German Aluminium Balloons." (Illustrated.)

19. *Daily Mail*.—"Shall Man Soar." Describing Mr. Hargave's soaring model.

20. *Golden Penny*.—"Cycling in the Air." Illustrated account of Myer's "Sky Cycle."

June *Strand Magazine*.—"Switzerland from a Balloon," with a series of interesting photos taken by Capt. Spelterini.
Pearson's Magazine.—"Ballooning after Shooting Stars." (Illustrated.)

3. *Golden Penny*.—"The Latest Attempts to Fly." Hargrave's, Herring's, and Danilwsky's.

7. *Le Temps* (Paris).—M. Don Simoni's Aeroplane Balloon. (Illustrated.)

23. *English Mechanic*.—"Cellular Kites."

Applications for Patents.

(*April, May, June*, 1899.)

7,264 April 6. C. A. CHAPPELL, London. **Improvements in or Relating to Aerial Vehicles.**

7,437. April 8. H. H. LAKE (Prof. E. Giampietro, Italy), **Improvements in Aerostats**

7,442. April 8. D. M. BOWYER SMYTH, London. **Improvements in or Relating to Propelling Means Suitable for Use in Flying Machines.**

7,482. April 10. J. MUNRO, Croydon. **Improvements in Aeronautics.**

7,635. April 11. A. RIEDINGER, Liverpool. **Improvements in Captive Balloons.**

7,809. April 13. T. FRITSCH, Liverpool. **Improved Apparatus for Use with Flying Machines.**

8,019. April 17. A. H. P. BLUNT, London. **Improvements in Flying Machines.**

9,238. May 2. P. M. JUSTICE (Zimmerman Flying Machine Company, United States). **Improvements in Tailless Kites.** (*Complete.*)

9,596. May 6. D. M. BOWYER SMYTH, London. **Improvements in or Relating to Propelling Means Suitable for Use in Flying Machines.**

10,443. May 17. O. O. DOMON, London. **Propelling and Steering of Balloons and Aerostats.**

11,293. May 30. A. J. BOULT (L. E. de Manresa, Spain). **Improvements in or Relating to Aerial Navigation.** (*Complete.*)

11,508. June 2. T. MOY, London. **Improvements in or Relating to Means and Appliances for Effecting Aerial Navigation.**

Patents Published.

131. January 3. **Aerial Machines.** ZEPPELIN, G. F. VON, 19, Keplerstrasse, Stuttgart, Germany.

The aerostat comprises a framework of longitudinal tubes *r* interspaced by wire ropes *s*, covered with wire gauze or netting, and with an outer covering of silk. It is made rigid by vertical struts *v* joined by inclined stays, and by circum-

ferential hoops. It is divided into compartments containing large gas bags which nearly fill the compartments and small ones from which gas can be expelled by the expansion of gas in the large ones. It is very long compared with its diameter, and supports two cars containing driving-gear for the propellers t; a gangway l runs the whole length of it from which any part of it may be reached by ladders f. Several aerostats with cars containing goods may be coupled together and to a motor balloon by universal joints, and their cylindrical surfaces are joined by fabric. Horizontal steering may be effected by rudders q, and the inclination of the axis is adjusted by moving a weight b, which is suspended from a wire rope, along which it is made to travel by an endless band actuated by winches in the cars. The ropes b^2 are wound on fusees on the winches which always keep them tight. The weight may be replaced by a couple of nailing ropes. The weight or load between the several aerostats or balloons of a series may be adjusted by transferring sand, liquids, &c., from one to another through flexible tubes.

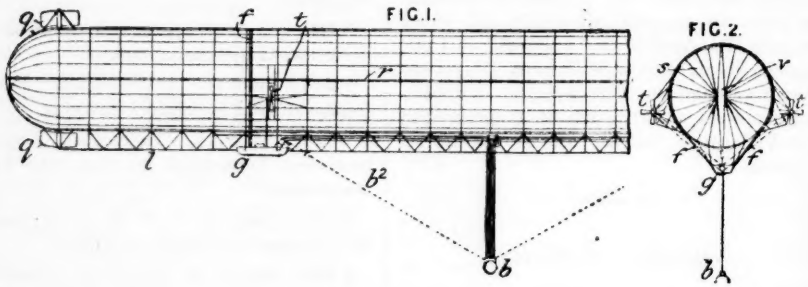
French Patents.

284,059. CZARNIAKOWSKI. **Dirigible Aerostat Based on the Natural Atmospheric Pressure displaced by the application of Centrifugal Force.**

284,142. DE MANRESA. **New system of Aerial Navigation.**

284,331. MONPELAS DE DAX. **Compressed Air Propeller for Dirigible Balloons.**

284,713 MARY. **Train Aerostat.**



284,889. MALEZIEUX **New system of Propulsion and the Direction of Aerostats.**

285,205. ZARSKI. **New Disposition of Platform for Dirigible Balloons.**

285,269. SUTER. **Dirigible Aerial Vehicle.**

United States Patents.

(*April, May, June*, 1899.)

Nil.